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ON FORMICID NOMENCLATURE

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For a number of years the author has had under consideration certain proposals which might aid in simplifying our unwieldy system of formicid nomenclature. The need for improvement has been evident to all who have struggled with our intricate pentanomial designations. I had supposed, however, that myrmecologists themselves understood the system well enough to avoid being confused by its complexity. I am no longer sure that this is the case since there has come to my attention a short paper from the pen of A. C. Cole¹ which indicates that the present status of formicid nomenclature is so intricate that even some students of ants fail to appreciate its varied ramifications. Thus Cole confounds the obvious need for nomenclatorial simplification with what he seems to regard as inherently chaotic taxonomy. I can put no other interpretation upon several of his statements, one of which is as follows:

“It is apparent that in order to prevent the classification of Formicidæ from attaining a more chaotic condition, the rules of nomenclature must be followed more conscientiously. The rather vague and unsatisfactory methods of the past must be discarded and replaced by thoroughly scientific procedure.”

¹ Cole, A. C. “Suggestions Concerning Taxonomic Nomenclature of the Hymenopterous Family Formicidæ, etc.” The Amer. Midland Entomologist, Vol. 19, No. 1, p. 236-241 (1938).

It is no purpose of mine to appear as an apologist for formicid taxonomy. Anyone who has taken the trouble to familiarize himself with the field will appreciate that it is in little need of defense. It may be truly stated that there are few other families of insects in which conservative taxonomic practice has been more generally or more carefully followed. It is largely because of this hyperconservatism that our present nomenclatorial difficulties have arisen. But, while it may be agreed that our nomenclature is involved, this by no means implies that the taxonomy on which it rests is chaotic. On the contrary it is obvious that students of ant taxonomy, in common with those who deal with other families of social insects, enjoy advantages inherent in social organisms. The inevitable variability of any long series from a single nest makes for conservatism in dealing with variation. When it is so readily apparent that the offspring of a single female may show considerable variation or, in the polymorphic species, may be wholly unlike, a natural check is placed upon the evaluation of differences which appear in new material. Furthermore the large series available to the descriptor has facilitated the exchange of type material. There is seldom an occasion where the author of a species cannot supply other workers with cotypes from the original nest series. The stabilizing effect on the field is too obvious to require comment. In the face of such facts anyone who contends that ant taxonomy is chaotic is merely demonstrating his ignorance of the true conditions.

I further find myself at odds with Cole in his contention that myrmecologists have violated the rules of nomenclature. This opinion is implied in the paragraph quoted above but finds full expression in the opening lines of Cole's paper which I quote:

"The writer is convinced that authors of descriptions of new ants who deal with categories less than species have very often violated our accepted rules of nomenclature. The status of a great many described kinds of ants is definitely insecure at present because of these inaccuracies."

It may be admitted that the use of infra-specific names in myrmecology is a peculiar one but this does not justify Cole's charge. Carlo Emery appears to have been the first to employ two infra-specific ranks with the variety subordinated to the subspe-

cies. I take it that this is the practice to which Cole objects. It may be pointed out, however, that there is nothing in the International Rules of Nomenclature that forbids such procedure which was an accepted rule of myrmecological nomenclature years before the International Rules were adopted. Nor can I see where this method is inaccurate. Emery was an exceptionally careful observer and he appreciated the fact that variations which occur within a species are not always of the same degree of magnitude. Emery therefore used the term variety to apply to very minor differences and the term subspecies to apply to the differences of somewhat greater magnitude. If this method is inaccurate I count myself no judge of accuracy. The question of its expediency from a nomenclatorial standpoint is wholly another thing. It is this aspect of the matter that I wish to discuss in the present paper.

Formicid taxonomy is usually spoken of as a pentanomial system because it employs four categories below the genus. These are subgenus, species, subspecies and variety. It might with equal propriety be called a hexanomial system for, in addition to the four recognized categories just cited, there is a fifth known as the "group." As yet the group has not made its appearance in formal nomenclatorial designations for the obvious reason that it is always designated by a specific name. Thus we have the *rufa* group in the genus *Formica* which may be distinguished from the *sanguinea*, *microgyna*, *exsecta* and *fusca* groups in the same genus. This arrangement is valuable for indicating relationships and, since it does not add any burden to the nomenclature, there is no reason to discard the practice. Similar considerations apply to the subgenus which may be employed in consonance with an abbreviated generic notation that does not add undue nomenclatorial burden. As the subgenus and the group are by no means myrmecological monopolies I take it that taxonomists in other fields have also found these groupings useful. On the other hand I believe, and trust, that our method of handling the subspecies and the variety is a unique taxonomic phenomenon. Although as originally envisaged the plan for the use of two infra-specific ranks was wholly conservative the unforeseen end result has been to complicate the nomenclature to such an extent that conservatism seems

apt to perish in a situation of its own devising. It may, therefore, be instructive to follow the steps by which this singular situation arose. The introduction of infra-specific ranks into the field of myrmecology is the joint contribution of Emery and Forel. Prior to the appearance of these two workers formicid taxonomy had been dominated by Gustav Mayr, a man whose invaluable contributions have been overshadowed by the more voluminous writings of his successors. Mayr's ability for generic delimitation has never been surpassed and it may be added that as early as 1861 he was able to produce a very creditable system of classification covering most of the European ant fauna without recourse to a single infra-specific name. This condition was not to last. It is difficult to ascertain the first appearance of the infra-specific unit. It is certain, however, that by 1874 Forel had definitely embarked on a course of specific subdivision. His *Fourmis de la Suisse*, which appeared in that year, lists numerous "races" assigned to species with which they showed relationship. Emery was not slow to follow Forel's lead and at first used the same term to apply to such forms. About 1885, however, Emery threw over Forel's term race and employed a new but equivalent one, the variety. Up to this point no particular difficulty in nomenclature had arisen. All infra-specific units, whether races or varieties, were assumed to be of equal rank. A trinomial was, therefore, the limit to which terminology could go. In the early 1890's Emery began to draw a distinction between such units. He introduced a new rank, which he called the subspecies, and discriminated between the status of this form and that of the variety. The effect of this novelty upon Forel was at once noticeable. Although he did not accept Emery's term subspecies for more than ten years he immediately recognized the existence of a second infra-specific rank. Forel appears to have had a soft spot in his heart for his own term race. At least he continued to employ it to apply to any infra-specific variant. Thus in his writings from 1895 to 1905 there are numerous references to races which are varieties and races which are "stirps." This last was a term adopted by Forel to designate the subspecific rank. At length Forel was won over to Emery's terminology and, since Wheeler had followed Emery's plan from the start, by 1910 we find the three dominant

figures in the field of myrmecology supporting the present system of infra-specific nomenclature. Support in this case did not mean that the system was accepted as wholly satisfactory. In 1913, when Wheeler published his book *Ants*, he discussed the shortcomings of the arrangement and in that volume followed a practice which was, in effect, a return to binomial nomenclature. Two years later Donisthorpe in his book *British Ants* reduced his nomenclature to a trinomial status by elevating subspecies to specific rank. Under other circumstances such efforts might have produced more general results. That they did not is unquestionably an outcome of the publication of the formicid section of the *Genera Insectorum*. Throughout this colossal work Emery adhered to the pentanomial system. The magnitude of his contribution was so great that further dissent was stifled. Hence we find myrmecologists struggling to control a situation which with each succeeding publication becomes more unmanageable.

Much has been claimed for the supposed phyletic value of the pentanomial system. The worth of this claim appears to have been considerably overestimated. Although the following remarks are restricted to the three lower categories, the species, subspecies and the variety, they will apply with equal cogency to the higher categories as well. When we write *F. rufa* subsp. *integroides* var. *coloradensis* Wheeler how much phylogeny is expressed? Not a great deal, for all that we are doing is to give a very partial view of a much more intricate series of relationships. True the system shows that the variety *coloradensis* "belongs" to the subspecies *integroides* but this, in itself, is apt to create an erroneous impression. Rather we should say that the subspecies *integroides* exists only as a combination of the seven varieties of which it is composed. Bear in mind that each of these varieties is supposed to show the same subspecific characteristic which delimits the group and, hence, all are coequal as regards this character. It may be admitted that one variant bears the distinction of having been first described. It is therefore not usually given its full name which would be subsp. *integroides* var. *integroides* but is simply designated as the "typical" *integroides*. Typical of what? Surely not of any phyletic consideration for in this particular there is no typical form. All seven variants are phyletically equal and to

demark one of them on the purely taxonomic basis of priority is to destroy whatever phyletic value the arrangement may have. Yet I do not believe that I am overstating the case when I say that after years of standing such "typical" subspecies acquire, in the minds of many myrmecologists, the status of stem forms. The same considerations apply to the relationship between subspecies and species. There are eight described subspecies clustered under the sheltering wing of the "typical" *rufa*. Unless one stops to remember that the "typical" *rufa* itself constitutes the ninth subspecies in the complex and is "typical" only because Linnaeus happened to describe it before any of the others were recognized, a faulty phyletic impression results. I contend that the pentanomial system is more apt to cause phyletic confusion than the reverse.

If we admit, as I think we must, that the pentanomial system is undesirable both from the standpoint of nomenclature and phyletics what remedial measures are possible? We may follow Donisthorpe's lead and elevate all the subspecies to specific rank. It may surprise those who regard the late W. M. Wheeler as a champion of the pentanomial system to learn that he favored this method. In 1935 he wrote me as follows:

"Of late I have been trying to get rid of many of the subspecies and varieties of ants by elevating them to species and subspecies, kicking them upstairs so to speak, largely because the nomenclature is becoming too complicated."

While one must heartily agree to the truth of this last statement it may be questioned that the proposed solution is entirely satisfactory. It places a value on the subspecies which some myrmecologists would hesitate to accept. I do not doubt that both Wheeler and Donisthorpe had in mind the supposed "specific incipience" of the subspecies. It has been held that subspecies are a sort of embryonic species which can be hatched into the adult condition either by natural selection or by the help of the taxonomist. For a number of reasons this latter sort of incubation seems more apt to be effective. Not the least among these is the utility of this rank as a repository for questionable forms. Into it could be dropped those confusing variants whose status as separate species might be called in question. As "incipient species" they

could remain subspecies indefinitely or, if more mature consideration showed them to be defensible as species, they could later be elevated to that rank with perfect propriety. I suppose that every myrmecologist has resorted to this method at times but one should look for better reasons than this before putting much stress on specific incipience as a basis for elevating subspecies. In this connection I would like to call attention to the fact that, although the subspecies has been in use as a rank in ant taxonomy for more than forty years the merest handful of forms have been raised from this rank to specific status. It may be argued that the time was not ripe for such taxonomic translation but this is not, in my opinion, the real reason. For all his conservatism the myrmecologist has never been loath to describe new species when these are sharply defined. We may grant that a considerable personal factor can enter here. This in no way weakens the argument. Whatever the personal equation governing specific delimitation may be, the description of a subspecies is proof that it shows a closer relationship to some other form than the describer would permit in the case of a species. Unless I am sadly mistaken by the time that this sort of incipience blossoms into full specific status there may be no myrmecologists to record the fact. Nor do I believe, even if we take the subjective viewpoint, that the elevation of the subspecies will give permanent nomenclatorial relief. If we accord specific status to the subspecies this automatically creates a superspecies. Let me illustrate what I mean. There are nine described subspecies in the case of *Formica rufa* L. The differences by which they may be separated are small variations of proportion, pilosity and color. Suppose that we kick all nine of these subspecies upstairs and make each a species. What shall we then call *F. foreliana*, *ciliata*, *dakotensis* and the other species which are now included in the “*rufa* group”? The relationship of each one to *rufa*, as long as *rufa* is a complex, is logical and satisfactory. Their cospecific status with the elevated subspecies of *rufa* would be quite another matter. With the “group” already enjoying a sub-rosa existence in formicid classification can anyone doubt that it will soon develop into another taxonomic rank if we elevate the subspecies?

Of recent years I have ceased to worry about subspecies as incipient species because of the greater interest which attaches to them as geographical races or choromorphs. In this I claim no originality for the idea has been repeatedly advanced by myrmecologists. Indeed one may point out that when Forel set about using the infra-specific unit for the first time it was as a geographical race. We cannot too much regret that the clarity of Forel's original stand, based as it was on intensive field work, should have been stultified by his subsequent acceptance of Emery's two sub-specific ranks. Once Forel became enmeshed in this system his earlier views concerning the choromorph suffered considerable damage. It seems certain that no small part of this was due to his increasing preoccupation with cabinet specimens whose exotic sources prevented field analysis. Adequate studies of this sort have been rare in myrmecology. This may surprise those who rightly regard the myrmecologist as an active field worker. There is a world of difference between taking specimens in the field and zoögeographical analysis. The first requires mainly patience, the second demands a working knowledge of previously described variants plus a highly organized survey type of collecting which must cover hundreds or thousands of miles in the field. It is not surprising that the results of the two methods differ. A beautiful example of this is to be found in W. M. Wheeler's two papers on the Australian genus *Leptomyrmex*. The first of these, published in 1915, was based largely on cabinet specimens. Wheeler had, it is true, collected a few forms in Queensland and New South Wales but the records based upon preserved material from other sources outnumbered these six to one. In this first paper Wheeler described a number of new varieties several of which were known from a single small nest series and one or two from unique specimens. As to what these varieties represented Wheeler made no attempt to state. His second publication on *Leptomyrmex* appeared in 1934. In 1931-2 he had spent considerable time in Australia and had made a much more extensive first-hand acquaintance with these insects. In this paper his personal field records constitute a third of the total. Moreover he had the advantage of his previous knowledge of the group. It is, therefore, not surprising that the introduction of the second paper carries the following remarks:

“At the present time 14 species are known. Several of them exhibit well marked color forms which Emery, Forel and I regarded as ‘varieties.’ Their constancy and local distribution, however, have convinced me that we are really dealing with distinct races or ‘Formenkreise.’ I have therefore raised all these varieties to subspecific rank.”

I am convinced that this concept will apply to most if not all of the valid infra-specific variants. I believe that adequate field studies on such complexes will show that their constituents, whether subspecies or varieties, are choromorphs. I am certain that this is true in the case of a number of infra-specific complexes in the genera *Formica*, *Pogonomyrmex* and *Aphaenogaster*. It seems to me that this suggests a very sound treatment for nomenclatorial simplification. If most subspecies and varieties are choromorphs why need we longer attempt to distinguish between the two ranks? Such a separation has always rested on an auctorial basis which cannot be subjected to analytical evaluation. The merging of the two ranks does away with this difficulty and promotes the concept of the infra-specific unit as a zoögeographical entity. As such its status can be tested by field observation. The nomenclatorial gain is too obvious to require comment; the most troublesome term of all is thereby relegated to limbo. In addition the present specific *status quo* is preserved in such a manner that no need for additional ranks should be felt. In opposition the worst that can be said is that the change gives too much prominence to a number of variants of doubtful validity. This is a fault of the describers and not of the suggested change.

I propose, therefore, that myrmecologists continue to exercise the conservatism for which they are famous and give over a taxonomic practice based largely upon auctorial evaluation for one more susceptible to factual proof. Let us reduce all infra-specific variants to a single rank, the subspecies, and thereafter eschew all temptation to return to varietal description. Most important of all let us refrain from describing additional infra-specific variants unless these have first been validated by adequate field observation.